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OCEANOGRAPHY—*Ocean-surveys: Problems and developments.*[1]

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Tradition has decreed that the address upon this occasion shall deal more or less with the work with which the outgoing president has been associated. So tonight we shall discuss the problems and developments of ocean-surveys with which we have been connected for over 23 years.

We usually think of the ocean as a vast moving highway, without visible paths or sign-posts. It occupies so large a part of the Earth's surface that knowledge of its contents and physical conditions is of prime importance. Especially is this true for certain problems relating to the physics of the Earth as a whole—geophysics. Human life and its environment and the evolutionary processes in the living world are influenced in countless ways by the varying physical properties of the ocean.

In the geophysical sciences with which we are concerned tonight, terrestrial magnetism, terrestrial electricity, and oceanography, much information has been collected already, though systematic investigations are but fairly started and the vast extent of the ocean still leaves many unsolved problems.

Our knowledge of the origin of the Earth's magnetic and electric fields is still imperfect. The exact interrelation between these two fields is not known, and we are interested in securing more information regarding the close connections which seem to exist between variations

[1] Address of the retiring President of the Philosophical Society of Washington, January 7, 1928, based on lecture *Purpose and progress of ocean-surveys* at the Carnegie Institution of Washington, November 22, 1927 (published in Scientific Monthly for February 1928). Received January 14, 1928.

in magnetism, atmospheric electricity, auroral displays, earth-currents, and transmission of wireless waves. Observations over the comparatively undisturbed ocean-areas will very materially aid in the discussion of all these phenomena.

The history of ocean magnetic-surveys goes back to the time of Edmund Halley who made the first observations in the Atlantic Ocean during 1698 to 1701. Before the invention of accurate timepieces, navigation consisted of observations of the noon altitude of the Sun for latitude, and crude estimation of course and distance to determine longitude. This method was very unsatisfactory, since unknown ocean-currents and errors in reckoning made it impossible to keep accurate account of the ship's position.

Large prizes were offered for a more reliable method of determining longitude, and in 1698 Halley began his voyages in the Atlantic Ocean on the PARAMOUR PINK to "improve the knowledge of the longitude and variations of the compass." As a result of these voyages he constructed and published the first magnetic chart of the oceans, and his method of drawing lines through points of equal declination is still used in nautical charts today. His map was used for many years, not to determine the longitude, the purpose for which it was constructed, but to give the navigator his compass correction as he sailed from port to port.

The changes which have taken place in the compass variation during the more than 200 years which have elapsed since the time of Halley are shown by comparing the early maps with those of the present day. Halley himself knew of these changes and cautioned the users of his chart to take them into account.

To give some idea of the amount of these changes, it might be noted that the cruises of the CARNEGIE in the Indian Ocean in 1911 and 1920 showed that the compass was changing its direction over one-third of a degree annually in the central part of the ocean. In 1911 the navigational charts for this ocean were in error by as much as one-half point chiefly owing to lack of accurate information as to the amount of the annual change. In 1580 the magnetic needle pointed 11° east of north at London and by 1812 it pointed 24° west of north, a change of 35° in 232 years. It now points only about 16° west of north. The causes of these changes and variations are not as yet known, and their explanation constitutes one of the chief problems in the science of terrestrial magnetism.

It is with these changes that we are chiefly concerned in a study of the earth as a magnet in trying to explain the origin of the Earth's

field, the causes of the changes which are observed during the day, the variation with the seasons during the year, and the secular or progressive change from year to year. We also wish to know why there is such close relation between changes in terrestrial magnetism known as magnetic storms and the occurrence of polar lights or auroras, both north and south, and changes in solar conditions, and why we have 11-year periods in magnetic changes coincident with the well-known 11-year periods in sunspot activity.

Since we cannot bring the Earth into the laboratory to study its problems, we must go out over its surface, penetrate into its interior, into its atmosphere, and into the ocean depths as far as present inventive genius will permit, and observe and record the results of experiments which nature is performing on a cosmical scale.

In order to secure the data necessary for a complete study of these various problems in magnetism, it was decided early in the organization of the magnetic-survey work of the Carnegie Institution of Washington to extend the investigations to the large ocean-areas. Since the time of Halley in 1700, occasional magnetic observations have been made at sea incidentally on voyages of discovery and exploration, such as those of the EREBUS and TERROR, the PAGODA, the CHALLENGER, the DISCOVERY, and the GAUSS. But over 200 years elapsed after Halley's survey before another expedition started out primarily to make magnetic observations at sea. The GALILEE sailed out of the Golden Gate in 1905 to survey the Pacific Ocean, making three cruises during the period August 1905 to June 1908. On these expeditions, the fruition largely of the plans and vision of Bauer and his colleagues, observations were made not only to determine the magnetic declination but also the magnetic dip or inclination and the strength of the Earth's magnetic field. The instruments used in these observations were mounted on an open bridge exposed to wind and weather, and many doubted whether worth-while observations could be made at sea with equipment then in use.

The accuracy of the results with these more or less experimental instruments and the promise of increased accuracy with new devices invented and constructed by Peters and Fleming indicated the desirability of continuing the ocean-survey. At the same time the effect of what little iron was present in the hull of the GALILEE was so difficult to control and measure in the results that it was decided to construct a specially designed nonmagnetic vessel. In 1909 the CARNEGIE was completed and began her long series of cruises in August of that year. Improvements in the instrumental equipment increased

materially both the amount and the accuracy of the data secured. Since advancement and success of ocean-surveys are measured by progress in the development of instrumental equipment, full credit must be given to those who have developed this phase of the ocean-surveys executed on the GALILEE and the CARNEGIE. Tribute also should be paid to the individual members of the several expeditions, who cannot be mentioned by name, but whose chief compensation is the thought that they have helped to add something of permanent value to science and to human knowledge.

A very satisfactory distribution of stations has been accomplished, and the accuracy of present-day magnetic charts used by navigators has steadily increased since the various hydrographic offices began using the data resulting from these surveys.

At only about 80 stations do we have cruise-intersections where reliable information has been obtained regarding the changes which have taken place over a period of years, the so-called secular variation. To improve this condition, in future ocean-survey work it is planned to retrace previous cruises and reoccupy as many points as possible in order to secure the maximum of data on the amount and direction of this secular variation. Valuable information for keeping navigation charts up to date, as well as the necessary data for the advancement of theoretical studies and investigations will thus be furnished. Future cruises need not be repeated at frequent intervals, and may be supplanted altogether by more rapid and efficient methods in connection with upper-air travel and research.

The study of the Earth's electric field, that is, of terrestrial electricity including both atmospheric electricity and earth-currents, is now being carried forward side by side with the study of the Earth's magnetism. The importance of these investigations has increased in recent years because of the close relation between variations in atmospheric-electric and earth-current phenomena and variations in magnetic conditions. Recent theories regarding the nature of electricity and the constitution of matter and the rapid advances made in radio transmission have given added stimulus to the study of the earth's electric field. The Sun is included in this study because of the close connection between magnetic and atmospheric-electric phenomena and solar activity, and coöperative work with the Mt. Wilson Solar Observatory in these investigations was started last year.

Some experimental observations in atmospheric electricity were made in 1908 on the GALILEE and in 1909 to 1914 on the CARNEGIE, but it was not until 1915 that a systematic and definite program of

observations was undertaken with new and improved methods and instruments devised and constructed chiefly by Swann and Fleming. A well-known European student of atmospheric electricity states that the only new contribution to this science within the past ten years was that resulting from the cruises of the CARNEGIE, which is especially valuable because of the wide distribution of information obtained.

The electric elements which have been investigated include potential gradient, both positive and negative ionic content, conductivity, and ionic mobility, penetrating radiation, and radioactive content of the air. The potential or electric charge in the air increases with height above the earth's surface, being about 100 volts at the height of one meter. This is the so-called potential gradient and is measured by noting the deflection on the fibers of an electrometer while the collector is raised one meter. There are present in the air at all times both positively and negatively charged particles called ions, about 1,000 of each kind in a cubic centimeter of air, and with our instruments it is possible to count the number with fair accuracy. Intimately connected with the number of ions in the air is the electric conductivity, or its ability to carry an electric current. Air is forced past a charged conductor at a uniform rate of speed, and the rate of discharge is noted by the changing position of the fibers in an electrometer.

Whether penetrating radiation or "cosmic rays" coming into the Earth's atmosphere from outer space can be one of the causes of the ionization of the air is one of the problems being investigated, and observations are made at sea to determine the amount and variation of this radiation by observing the rate of ionization in a closed copper vessel. The radioactive-content observations are arranged to collect and measure the amount of radioactive material, such as radium and thorium, present in our atmosphere, this being another source of ionization.

Under the action of the Earth's electric field, positive ions are traveling toward the Earth and negative ions are traveling upward in the air, giving rise to an air-earth electric current. The rate at which this interchange takes place would neutralize the earth's negative charge in a very short time were there no recharging source of energy. Various theories have been advanced to account for the source of this supply, e.g., lightning or the Sun, but the problem still awaits solution.

Conditions at sea are much more favorable for investigating the Earth's electric field than on land, where dust and smoke in the air and presence of changing cultural or permanent topographic features

such as trees, buildings, or contours cloak the true characteristics of the atmospheric-electric phenomena. To improve further the atmospheric-electric results and to note the conditions at sea, dust-count observations are to be included in the program since it is known that the presence of dust in the air has a marked influence upon atmospheric-electric conditions.

In order to determine the variations which take place in the elements of the Earth's electric field during a 24-hour period, continuous observations of these changes are carried out on the CARNEGIE at frequent intervals. I will mention only the difficulties of carrying out such observations under conditions which persist at sea, insulation troubles due to condensation and to salt spray, and difficulties attending the use of instruments on a rolling ship. In dealing with insulation difficulties we have learned that our favorite slogan "Electricity never fails" has always held true. The instrument will always work if the insulation surfaces are clean and all connections are good.

A discussion by Mauchly of these 24-hour series of atmospheric-electric observations at sea on the CARNEGIE disclosed that the chief maximum of the diurnal variation of the potential gradient occurs at about 18^h Greenwich mean time all over the world, approximately the time when the Sun is in the meridian of the north magnetic pole. This conclusion was confirmed by Sverdrup during Amundsen's Arctic-Drift Expedition on the MAUD. The true physical explanation of this discovery is not yet apparent. Wait and Sverdrup point out that the rotating magnetic field of the earth induces electromotive forces in the earth's electric field, the variations of which are in remarkable agreement with the observed variations of the potential gradient over the oceans. This agreement appears too good to be accidental, but further evidence is needed for a satisfactory physical interpretation. To add to the information regarding variations of the potential gradient, an automatic photographic recording electrometer is to be mounted near the truck of the mainmast during the next cruise. Some experimental work with this apparatus was done during the return of the CARNEGIE from New York to Washington last month by W. C. Parkinson, who will have charge of the atmospheric-electric work during the next cruise.

The important contributions to the study of various geophysical problems which are being made by investigations of the Kennelly-Heaviside conducting layer and of radio transmission and variations with changing magnetic and electric conditions greatly enhance the value of the ocean atmospheric-electric data and indicate coöperative

investigations along similar lines for future ocean-work. It is planned to begin at sea investigations of the conducting layer and to carry out experiments on the variations of signal-intensity, following methods already in use on land.

Important correlations between earth-current variations and changes in other geophysical and cosmical phenomena, such as solar activity, magnetic disturbances, and polar lights, have resulted from an investigation of observatory records, and the importance of these discussions in the general study of the Earth's magnetic and electric fields now warrants beginning systematic earth-current observations at sea. Some preliminary experimental work was done by O. H. Gish while the CARNEGIE was en route from New York to Washington this year in order to determine the best methods and instruments for such investigations over the oceans.

The challenge of the vast, practically unknown, expanse of the atmosphere above the Earth's surface and of the equally unexplored depths of the ocean awaits the pioneering spirit of a Langley or the ingenuity of a Lord Kelvin to penetrate their mysteries. When inventive genius makes it possible to investigate the modifications in magnetic and electric variations due to change in altitude, many new and important discoveries will be made.

The mysteries of the ocean-depths, however, are slowly being unfolded through advances in the growing science of oceanography. Up to the time of the CHALLENGER expedition in 1872 to 1876, oceanographic research had been limited to restricted areas, or was incidental to some exploratory expedition, or was associated with some national fisheries investigations. Following the stimulus given by the CHALLENGER results, oceanographic investigations were much extended, and new methods and instruments were devised. However, the vastness of the regions to be explored and the time and expense entailed in sending instruments to the bottom of the ocean-deeps leave many unsolved problems for the oceanographer. Time does not permit more than a brief mention of the pioneer work done by Forbes, Thomson, Agassiz, Murray, and others who laid the foundations of the present science of oceanography.

In spite of all the vast amount of data that has been collected, we have only a general idea of the contours of the ocean-bed and only a meager knowledge of the bottom sedimentary deposits which are of peculiar interest to the geologist in his study of the age and formation of the Earth and the changes which time has witnessed. The mapping of the configuration of the oceanic basins covering over two-thirds

Fig. 1.—Tentative route for the seventh cruise of the CARNEGIE, 1928–31.

of the Earth's surface should be as important as the mapping of the land masses which occupy less than one-third. Such information is necessary for the geodesist in his study of the movements within the Earth's crust and for the seismologist in his study of the origin, history, and probable future development of submarine earthquakes.

The movements of vast bodies of water relatively to one another and to the land due to winds and tide, and the vertical movements due to changes of temperature and salinity make the ocean with its vast capacity for carrying heat a powerful factor in its influence upon practically every phase of life upon the Earth, in its control of climate, and in its determining effect upon man's migration and habitation.

Perhaps the most fascinating study connected with the sea is the multitudinous life found in all oceanic waters from the surface down to the deepest abyss yet explored. Physical changes in the ocean-waters have profound influences upon marine life, its variety, its amount, and its distribution. A knowledge of these influences will contribute in many ways not only to the study of evolutionary processes taking place in the sea but to the practical problem of economic use of the ocean's food resources.

Many problems of oceanography are of interest to the Carnegie Institution of Washington through the activities of its various departments and research associates. The vast extent of the ocean-areas to be covered by the next cruise of the CARNEGIE offers unique opportunity to add new and much-needed information in this science from regions never investigated.

To carry out the proposed increased program of general oceanographic work has required many structural changes on the CARNEGIE. During the past summer the vessel was in Hoboken, New Jersey, undergoing repairs and alterations. A new stateroom was added in the cabin since the technical staff is to be increased to seven, the additional man to be especially trained in chemistry and marine biology. The two lifeboats were moved from the quarter-deck to overhead platforms amidships opposite the after dome, leaving the quarter-deck free for the operation of the bronze winch, sounding wire, and special davits for handling tow-nets, water-bottles, deep-sea reversing thermometers, and bottom-samplers.

Two new laboratories were constructed on deck; one will be specially fitted for physical oceanographic, biological, and chemical work, and the other will house the radio and echo-sounding equipment.

In physical oceanography it is planned to obtain temperatures and water samples at depths of 5, 25, 50, 75, 100, 200, 300, 400, 500, 700,

1,000, 1,500, and 2,000 meters every 150 to 200 miles, with occasional series down to the bottom with a limit at 20,000 feet. To obtain a continuous record of surface-temperatures, a distant-recording thermograph has been installed with its bulb on the hull about seven feet below the water-line and with its recorder in the new laboratory. These records will be checked occasionally by the usual method, direct readings with draw-bucket and thermometer.

Water samples and temperatures will be secured by Nansen water-bottles and Richter reversing deep-sea thermometers using a series of ten on the wire at one time. The water-bottle has a capacity of $1\frac{1}{4}$ liters, and two thermometers will be used with each one in order to check the temperature. The salinity and density of each water-sample will be determined on board ship by the Wenner electric conductivity method and checked occasionally by the silver-nitrate titration method. The water-sample also will be analyzed for oxygen, nitrate, and phosphate content, and hydrogen-ion concentration.

Samples of muds and sediments from the bottom will be secured by use of the snapper-type of sampler, as modified by Vaughan, and a larger Eckman tube-sampler, as modified by Trask for deep water. It is now known that bottom-living creatures feed on organic matter found in bottom muds, and that these muds are often teeming with life. From a study of these organisms and fossil remains, together with borings from oil-wells, important conclusions have been reached regarding the origin of oil-producing deposits. The Geophysical Laboratory also is interested in the nature and derivation of inorganic marine deposits in the study of the age of the earth and the various processes of its formation.

The machinery necessary to handle water-bottles, thermometers, and bottom-samplers has been installed on the CARNEGIE. It consists of a 30-horse-power gasoline engine and a 12-kilowatt generator installed below decks in the engine-room to furnish the required electric power. A bronze winch weighing three tons, operated by a 15-horse-power electric motor, has been installed on deck. Two reels and two gypsy-heads are provided, one reel containing 20,210 feet of special aluminum-bronze stranded wire rope $\frac{3}{16}$ inch or 4 mm. in diameter, and the other containing 6,808 feet of $\frac{1}{4}$ inch or 6 mm. wire. This wire was made in Germany and was designed especially for oceanographic work after extensive tests and experiments by those in charge of preparations for the German Expedition on the METEOR, 1925 to 1927.

The gypsy-heads are to be used in handling yards, sails, hoisting lifeboats, hauling in earth-current cables, and for the general work on deck. Special bronze davits and blocks have been installed for

handling the wire as it is payed out or hauled in. Platforms have been constructed on both port and starboard sides, where the observer will stand while attaching or detaching the water-bottles and thermometers to the sounding wire.

The winch has been constructed so that the reels may be operated either singly or together, thus allowing one wire to be payed out on the brake while the other wire is being hauled in. This will allow two series of water-bottles to be operated simultaneously, thus saving materially in the time required at each station.

It is planned to heave to every other afternoon, taking in all sails except those required to keep the vessel as nearly stationary as possible. To keep the wire vertical or nearly so will require skilful maneuvering, and it may be necessary to use the main engine occasionally to accomplish this result. Helland-Hansen and Nansen have carried out similar work with great success on the ARMAUER HANSEN, a sailing vessel of only half the size of the CARNEGIE. They state that[2] "owing to its special construction the ship is easily maneuvered in such a manner that the line along which the oceanographic instruments are suspended remains in a vertical position throughout the time of observation even if there is a strong drift caused by wind or current." This is essential in order to obtain correct depths by wire measurements.

As a further aid in checking the depths at which temperatures and water-samples are obtained, simultaneous use is to be made of protected and unprotected thermometers, calibrated for pressure-effects, placed at frequent intervals as the wire is lowered into the water. The difference between the readings of the two thermometers at any level will give the depth for that level. This method was used recently with success by the German Atlantic Expedition on the METEOR. To avoid rapid and excessive drift of the vessel when hove to in a strong breeze, sea-anchors will be used to check the headway. Simultaneous determinations of depths with actual wire soundings and with the echo-method, together with temperature and salinity data at all levels, will give information of great value in establishing proper formulae for the velocity of sound in sea-water in the deep basins of the ocean.

The latest type of sonic depth-finder as developed by Harvey C. Hayes has been installed on board the CARNEGIE, and frequent determinations of ocean-depths will be made as the vessel is proceeding on her way. The depth can be determined in a very few moments, the method consisting essentially of measuring very accurately the

[2] B. HELLAND-HANSEN and FRIDTJOF NANSEN. *The Eastern North Atlantic.* Geofysiske Publikasjoner 4: No. 2, 3–4. 1926.

time-interval between a signal sent out from the ship and the return of the echo from the bottom of the sea. The United States Navy is coöperating in this work by lending the necessary equipment consisting of an oscillator for transmitting the signal, six microphones for receiving the echo, and a depth-finder for measuring accurately the interval between signal and echo. The method is accurate to within about ±5 fathoms or 30 feet for depths greater than 100 fathoms, the range over which the sonic depth-finder is designed to operate.

In marine biology it is planned to confine attention to microbiology, to determine the abundance and distribution of plankton and other microscopic organisms. Shallow-water dredging for diatoms and foraminifera will be undertaken also in coöperation with Dr. Albert Mann, a research associate of the Carnegie Institution of Washington. Quantitative distribution of plankton at various depths from the surface down to 100 meters will be determined by the examination of definite quantities of water brought up by means of special water-bottles, or by use of a hose let down to depths found practicable.

Some study will be made also of surface plankton by straining a continuous stream of water through a fine-meshed net. Marine organisms also will be secured by tow-nets, and hauls both vertical and horizontal are to be made from the surface down to a depth of 150 to 200 meters, and occasionally to greater depths. A special boom-walk has been rigged in connection with the vessel's boat-boom, where the observer can walk out 30 feet from the ship's side in fair weather, dip up any marine life from the surface, or operate the tow-nets well out from the disturbing influence of the vessel and its motion through the water.

To assist the biologist, H. R. Seiwell, in his study of marine life in its native habitat at the bottom of the ocean, a diving helmet has been secured for use in shallow-water. This device has been used by amateurs at depths of 30 feet and can be used safely at depths of 50 to 100 feet.

Equipment will be carried also for securing specimens of dolphins and porpoises from regions where no specimens have been secured heretofore. Such specimens are of special interest to Remington Kellogg, a research associate of the Carnegie Institution of Washington, in his study of the evolution of the whale and other marine vertebrates.

Limited space on the vessel and time and restrictions as to power and machinery prohibit undertaking any deep-sea trawling or dredging. This work may be taken up during a later cruise when it is hoped that chief attention may be devoted to work in oceanography.

Any program of oceanographic investigations should include extensive work in marine meteorology in view of the important influence upon climate of mass movements of large bodies of heat-bearing oceanic waters. The study of the physical interchange between the surface of the ocean and the air above it is important in the study of atmospheric circulation and disturbance over the entire surface of the Earth because of the fairly normal conditions which exist at sea.

The foundations of the science of marine meteorology were laid by an American, Admiral Matthew Fontaine Maury. His book *Physical Geography of the Sea* is still a classic, although some of his theories and conclusions have been supplanted. Due to his efforts an international conference was held at Brussels in 1853, and a general program for marine meteorological observations was adopted. Maury introduced sailing directions and pilot charts which were of untold value to shipping interests, especially in the time of sailing ships. Monthly pilot charts of the great oceans are now issued in advance by the U. S. Hydrographic Office and the British Admiralty and constitute a most important aid to navigation.

While conditions at sea are fairly normal for the study of the atmosphere, yet the ocean is only a highway and the observer is always on the move from place to place. Instruments must be especially adapted for use on moving and rolling platforms, and progress in marine meteorology, as in other oceanographic investigations, has developed only as rapidly as the invention and utilization of the proper instrumental equipment has permitted.

To study the physical interchange of heat and moisture between the ocean and the atmosphere, it is planned to observe the temperature and humidity lapse-rates from sea-level to masthead. Accompanying observations of wind direction and velocity and of changes in atmospheric pressure will be made.

Variations in the amount of solar radiation received at the Earth's surface and their influence upon world-wide weather conditions have been the subject of much study in recent years by Abbot and Clayton. It has been thought worth while to include such observations in the meteorological program, together with observations of cloud systems, rainfall, evaporation, and dust-content and carbonic-acid content of the atmosphere. Additional data over the great oceanic areas to be covered by the next cruise of the CARNEGIE may be extremely valuable in the comparison of world weather with solar variation, in the determination of the rate at which the atmosphere is being charged with water vapor so vital to life on the continents, and in the study of the dynamics of atmospheric circulation over the oceans.

It is planned to compute at sea and publish promptly as the cruise progresses the pertinent oceanographic data for use of students and investigators of oceanography, as has been done heretofore in terrestrial magnetism. The physical data to be published include the following results of observations and calculations at various depths: Temperature, salinity, density observed and corrected for compression, oxygen-content, hydrogen-ion concentration, specific volume, and dynamic pressure and depth. A part of the water-samples will be tested for salinity and for gas-content, and a part will be stored below decks for later study in various laboratories. Biological specimens will be studied, sketched, and preserved for transmission to some museum.

The dynamic calculations will be made in accordance with the method devised by Bjerknes and as modified by Hesselberg, Sverdrup, and others. The dynamic conditions in the ocean may be viewed in the same light as similar conditions in the air. Winds blow obliquely from areas of high pressure towards areas of low pressure, taking into account the effect of the rotation of the Earth. The force or velocity is proportional to the gradient or difference in pressure. So in the ocean, data as to temperature and density at two points at the same level permit us to calculate the difference in dynamic pressure between the two points. This is one of the factors which cause circulation, and the direction of this circulation is affected by the rotation of the Earth.

In the biological and chemical work, chief emphasis will be placed upon the collection of data and specimens. Some analysis of water-samples and study of specimens must be done on board ship immediately after collection, and as complete a preliminary examination and report as possible of the results of these investigations will be made as the cruise progresses. Interested organizations will be furnished with water-samples, bottom-samples, and biological specimens for further study and report, and a final discussion and publication of the results of the cruise will be made by the Institution at the conclusion of the work.

The Carnegie Institution of Washington is indebted to the following institutions and organizations for coöperation by lending special equipment or by giving expert advice in planning the program of of investigations: United States Navy Department, National Museum, Bureau of Fisheries, Weather Bureau, and Coast and Geodetic Survey; Scripps Institution of Oceanography of the University of California; Museum of Comparative Zoology of Harvard University; School of Geography of Clark University; Geophysical Institute, Bergen, Norway; Marine Biological Association of the United King-

dom, Plymouth, England; German Atlantic Expedition of the METEOR; and Carlsberg Laboratorium, Copenhagen, Denmark.

In this brief discussion of ocean-surveys it is possible to present only a few of the outstanding developments and to sketch only briefly the problems as yet unsolved. The chief advances in surveys already made have come from invention of instruments capable of revealing the variations in natural phenomena in regions hitherto inaccessible. Thus the pathway to further progress in these branches of geophysics is made plain, and man's inventive genius is challenged in no uncertain terms.

PHYSICS—*The deformation of granular solids.*[1] P. G. NUTTING, U. S. Geological Survey.

Theoretical mechanics has long suffered from the lack of a function capable of representing the deformation of bodies possessing elastic, plastic, and viscous properties all at the same time. Many solids yield gradually and continuously to stresses far below their elastic limits and without losing their power of elastic recovery at any stage of strain. Solids strained beyond their elastic limits exhibit many plastic properties. Viscous fluids behave somewhat like solids above their elastic limits. The pitches studied by the writer[2] possess a remarkable combination of elastic and viscous properties. The mathematical difficulty is to represent a viscous flow in combination with elasticity in accordance with Hooke's Law.

I am indebted to Mr. W. W. Rubey[3] of the U. S. Geological Survey for geological data which led to a simple formula which in turn led by integration to a new deformation function of wide applicability having many remarkable properties.

Well borings, taken in compacted granular material of fairly uniform grain density, indicated the relation Depth × Void Ratio = Constant to within the uncertainty of measurement, allowance having been made for erosion. We have then $zR = C$ where z = depth and R, void ratio, is the ratio of void volume to that filled by grains. From this we have to obtain a relation between pressure and volume. Calling the pressure p, the (variable) density of the material ρ and the (constant) density of the grains ρ_g, we have

$$p = \int_0^z \rho \, dz$$

[1] Published by permission of the Director of the U. S. Geological Survey. Received January 14, 1928.

[2] Amer. Soc. Test. Mat. Trans. 1921; and Journ. Frank. Inst. May, 1921.

[3] Bull. Amer. Assoc. Petr. Geol. 11. 1927

$$\frac{\rho}{\rho_g} = \frac{1}{1+R} = \frac{1}{1+C/z} = \frac{z}{z+C}$$

Hence by substitution and integration

(1) $$p = \rho_g C(z/C - \log(1 + z/C))$$

a function the simplest form of which is

(2) $$ay = x - \log(1 + x)$$

Substituting for z/C, $1/R = \rho / (\rho_0 - \rho)$ in (1) gives

(3) $$p = \rho_g C \left(\frac{\rho}{\rho_g - \rho} - \log \frac{\rho_g}{\rho_g - \rho} \right)$$

a relation between pressure and density. Finally the relation between pressure and volume is obtained at once from (3) (since $v \doteq 1/\rho$)

(4) $$p = \frac{C}{v_g} \left(\frac{1}{v/v_g - 1} - \log \frac{1}{1 - v_g/v} \right)$$

Put in the form of an equation of state, this is

(5) $$\left(p + \frac{C}{v_g} \log(1 - v_g/v)\right)(v - v_g) = C$$

In this equation, $v - v_g$ corresponds with $v - b$ of van der Waals, $C = RT$ and $(C/v_g) \log(1 - v_g/v) = a/v^2$. (5) reduces to Boyle's Law pv = const. for large specific volumes as it should. For very large pressures, v approaches v_g, i.e. the granular material approaches a solid whose density is the grain density.

From (4) the compressibility $\beta = (dv/v)/dp$, is

(6) $$C\beta = v_g (v/v_g - 1)^2$$

a quantity approaching zero as the granular material approaches homogeneity. Eliminating C between (4) and (6) gives the dimensionless characteristic βp

$$-\frac{d \log v}{d \log p} = \beta p = (v/v_g - 1) + (v/v_g - 1)^2 \log(1 - v_g/v)$$

which is again a function of specific volume.

In the general equation between compressive and repulsive pressures

(7) $$p + P = \pi + \kappa$$

in which p = external (transmitted) pressure, P = cohesive pressure, π = distending (static) pressure—due to contact—and κ = kinetic (thermal) distending pressure, holding at any plane in any body, P and κ are negligibly small in loose masses of granular solids and $p = \pi$. In gases P and π are practically zero and $p = \kappa$. T. W. Rich-

ards finds P proportional to about the square and π to about the seventh power of the densities of many elementary solids and liquids over small ranges. These relations are special cases of (3) and (4).

Equation (3) between pressure and density may be used to compute grain density by eliminating C between two sets of values. This was done for water using the data of Bridgman, regarding the molecules as but small grains. The computed grain (molecular) density was 1.12 and porosity 10 per cent but both varied somewhat with pressure since the molecules are not incompressible nor are cohesive forces absent.

Such physical checks on the new function as have been given are serviceable since it rests on a purely empirical foundation. Its geometrical form is shown by Figure 1.

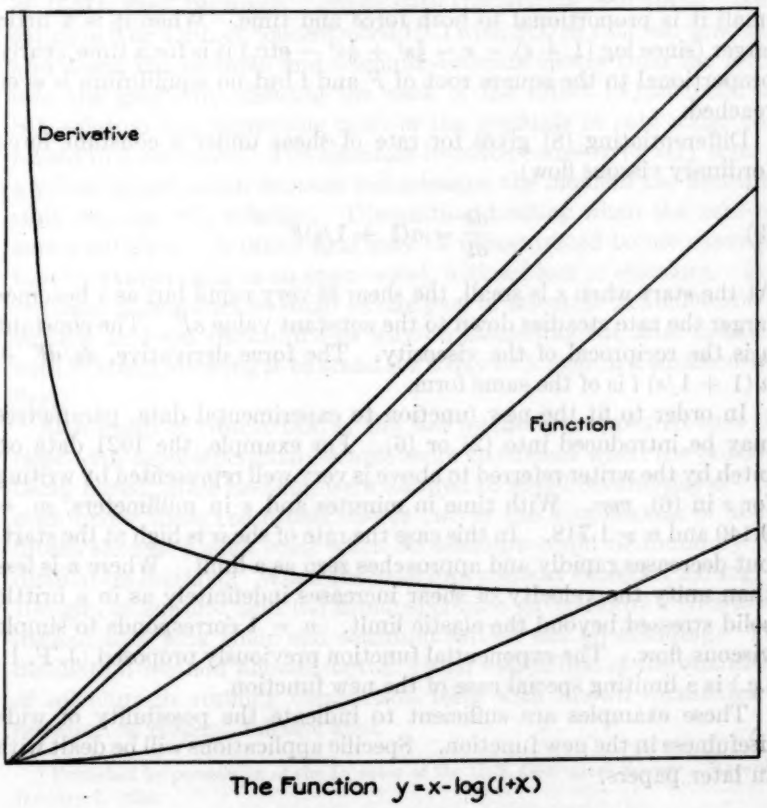

Fig. 1.—Graph of the function $y = x - \log (1 + x)$

The graph has the form of a tilted sled runner sharply curved near the origin but rapidly approaching a linear relation between the variables. In the figure, each curve is the toe of the preceding one magnified ten times.

These curves closely resemble those observed in the shear of very viscous fluids in the relation between pressure and shear. The initial curved portion of the graph has been attributed[4] to plastic properties and the straight line portion to true viscous flow. But evidently a smooth continuous function may represent both. Calling s shear, F force per unit area and t time, then

$$s - \log(1 + s) = aFt \tag{8}$$

will have the general characteristics desired. When the shear s is small it is proportional to both force and time. When it is a little larger (since $\log(1 + s) = s - \frac{1}{2}s^2 + \frac{1}{3}s^3 -$ etc.) it is for a time nearly proportional to the square root of F and t but no equilibrium is ever reached.

Differentiating (8) gives for rate of shear under a constant force (ordinary viscous flow)

$$\frac{ds}{dt} = a(1 + 1/s)F \tag{9}$$

At the start when s is small, the shear is very rapid but as s becomes larger the rate steadies down to the constant value aF. The constant a is the reciprocal of the viscosity. The force derivative, $ds/dF = a\,(1 + 1/s)\,t$ is of the same form.

In order to fit the new function to experimental data, parameters may be introduced into (2) or (6). For example, the 1921 data on pitch by the writer referred to above is very well represented by writing for s in (6), ms^n. With time in minutes and s in millimeters, $m = 0.140$ and $n = 1.718$. In this case the rate of shear is high at the start, but decreases rapidly and approaches zero as a limit. Where n is less than unity the velocity of shear increases indefinitely as in a brittle solid stressed beyond the elastic limit. $n = 1$ corresponds to simple viscous flow. The exponential function previously proposed (J. F. I., l.c.) is a limiting special case of the new function.

These examples are sufficient to indicate the possibility of wide usefulness in the new function. Specific applications will be dealt with in later papers.

[4] BINGHAM. *Fluidity and plasticity*, p. 217, *f.* 77, *81.*

CHEMISTRY.—*Examination of sulfuric acid for selenium.*[1] R. C. WELLS, U. S. Geological Survey.

In testing commercial sulfuric acid for selenium by means of aspidospermine according to the suggestion of L. P. J. Palet[2] it was found that instead of a pink color different shades and colors were developed, depending on the strength of acid and time of heating, so that the conclusions were somewhat uncertain. In order to check the results by an independent method the following scheme was devised and found to work very well.

Dissolve from 0.5 to 1.0 gm. of KBr in 3 or 4 ml. of bromine water and allow it to run through a funnel into a 200 ml. retort. Next add from 65 to 100 ml. of the sulfuric acid to be tested, so that about 100 gm. of H_2SO_4 shall be taken. Shake until the mixture is uniform. Next saturate 3 or 4 ml. of concentrated HCl with SO_2 in a 25 ml. graduate and distill the bromine and selenium bromide slowly from the retort into the graduate, allowing the neck of the retort to just touch the SO_2 solution and immersing most of the graduate in cold water contained in a casserole. The selenium bromide comes over very soon as a yellow liquid which deposits red selenium the moment the first drop runs into the SO_2 solution. Discontinue heating when the acid becomes colorless. A dilute acid may be concentrated before examination by evaporating in an open vessel, without loss of selenium. Percentages of selenium as small as one part of selenium in 10,000,000 of sulfuric acid are shown in this way on saturating the final distillate with SO_2 and allowing it to stand for a day or longer in a small corked flask.

As a result of comparison of the two methods it appeared that the tests with aspidospermine became more consistent when the portions being tested, after heating, were allowed to stand several days. Yet even under these conditions one or two of the samples out of about fifteen showed a pink color when no selenium could be found by the distillation method. When the selenium content exceeded 2.0 mg. in 100 gm. of sulfuric acid the aspidospermine gave a brown coloration instead of a pink color and selenium settled out on standing. The distillation method allowed better visual estimation of the quantities of selenium in running comparison tests with known quantities of selenium in reagent sulfuric acid.

[1] Published by permission of the Director of the U. S. Geological Survey. Received January 7, 1928.

[2] Annales de chimie analytique **23**: 25. 1918.

The method described above and depending on the volatility of selenium bromide was suggested by the procedure of Noyes and Bray,[3] but whereas they recommend the use of hydrobromic acid as a reagent and distill most of it, the distillation in the case of sulfuric acid does not need to be carried even to the point where fumes of sulfuric acid begin to go over.

PALEONTOLOGY.—*The generic name* Orbiculoidea *d'Orbigny and its application.*[1] GEORGE H. GIRTY, U. S. Geological Survey. (Communicated by JOHN B. REESIDE, JR.)

The name proper to the most common type of discinoid in the Devonian and Carboniferous faunas of America has been a subject of languid controversy for a number of years, and the present paper is in the nature of a reply to my friend, the late Prof. C. S. Prosser, and to the arguments that he advanced as long ago as 1912. The intervening years have removed Professor Prosser from his honored sphere, but the questions raised between us are still alive. Would it were otherwise. The following broad statements will supply a background for the discussion whose details will be filled in farther on.

Discinoids were fairly abundant in Paleozoic faunas, and they are also present in living ones, but during the intervening period they appear to have been relatively rare. Of species many had of course been named among the fossil forms and of genera a few had been more or less inadequately described, but up to the publication of Hall and Clarke's monograph on the Brachiopoda[2] most of the fossil species had been referred under the living genus *Discina*. Such authors as described new genera among these fossils or in turn sought to identify those genera scarcely did more than bring confusion into the subject. To these shells Hall and Clarke accorded treatment that seems particularly elaborate. They recognized a number of generic types and for the one which we are considering here they revived a half-forgotten name of d'Orbigny's, *Orbiculoidea*. This name thus received rather widespread acceptance.

In 1909 and again in 1911,[3] having occasion to describe certain shells of this type, I gave reasons why, as I thought, *Orbiculoidea* could not

[3] *A system of qualitative analysis for the rare elements* (Macmillan, 1927), p. 37.

[1] Published by permission of the Director of the U. S. Geological Survey. Received January 14, 1928.

[2] *Natural History of New York*, Pal. 8: Brachiopoda, pt. 1. 1892.

[3] U. S. Geol. Surv. Bull. 377: 18. 1909; Bull. 439: 37. 1911.

properly be used in the sense in which it was revived by Hall and Clarke, and I suggested that *Lingulidiscina* Whitfield appeared to connote the same general elements of structure; *Lingulidiscina* then might be substituted for *Orbiculoidea*. At that time and subsequently I employed the generic term *Lingulidiscina* in my own writing, but my proposal has had relatively little following, although I may fairly say that this fact reflects rather ignorance that any controversy existed or indifference to its outcome than opinion as to the merits of either name. At least no one so far as I am aware gave any reasons for employing *Orbiculoidea* in preference to *Lingulidiscina* or *Lingulidiscina* in preference to *Orbiculoidea* until Professor Prosser took up the argument in the paper above referred to.

Now there are obviously two distinct questions at issue, and these questions are measurably independent, or at least they depend on different facts and are susceptible to independent consideration. One is, whether *Orbiculoidea* is available as a generic name for this type of shell; the other is, if *Orbiculoidea* is not available what other name can be employed. Should the first question be answered in the affirmative, the second, of course, would cease to have any real importance.

The conclusions that I reached in 1909 rested on the following premises: (1) The brachiopod types that Hall and Clarke designated by the names *Orbiculoidea* d'Orbigny and *Schizotreta* Kutorga, constitute two distinct genera. (2) The name *Schizotreta* has priority of publication over *Orbiculoidea*. (3) The first species mentioned under *Orbiculoidea* (*O. forbesi*) is a "*Schizotreta*" while the second (*O. morrisi*) is an "*Orbiculoidea*." By application of the "first species" rule to these facts, *Orbiculoidea* became a synonym of *Schizotreta* and the group of species called "*Orbiculoidea*" by Hall and Clarke was left without a name, or at least it was bereft of the name *Orbiculoidea*. *Lingulidiscina* Whitfield seemed available to replace *Orbiculoidea* and I used it. I propose to examine the status of each one of these premises and also to glance at certain other considerations that bear upon the same issues.

The authentic meaning of the term *Orbiculoidea* revolves about the identification of its genotype and the characters that the genotype is found to possess. A search for the facts about these points is complicated by an extraordinary number of ambiguities and inaccuracies of statement. *Absit omen.*

The following is by Hall and Clarke from page 128 of the monograph:

> The definition of this term [*Orbiculoidea*] was first given in the Prodrome de Paléontologie Stratigraphique, 1849, and is in the following terms: "Coquille de contèxture cornée non perforée, dont la valve inférieure concave est pourvue d'un ouverture laterale ou crochet pour le passage d'un pedicule simple," the first example cited under this definition being the *Orbicula Morrisi*, Davidson. Mr. Dall has observed that in neither the first use of the term, nor in the second, was an example cited, and, therefore, reaches the unavoidable conclusion that *O. Morrisi*, being the first citation made by the author, must be assumed as the typical species.

Evidence is then offered for believing that *Orbicula morrisi* Davidson, had the same type of structure as the American shells with which we are now familiar under the term *Orbiculoidea*. In this way the name "*Orbiculoidea*" was brought into conjunction with the "very compact generic group" which we here have under discussion. Some of the facts employed in this chain of evidence, however, seem to be erroneous. Apparently at the time of writing the authors had not examined any of the three works of d'Orbigny mentioned, for in that brief paragraph they embody misstatements that would scarcely have found expression if they had consulted the original sources. As a matter of fact "the definition of this term" was not "first given in the Prodrome," the *Prodrome* was not published in 1849, and the first species mentioned under *Orbiculoidea* was not *Orbicula morrisi*. These errors were apparently recognized by the authors at some later date, for the following passage appears on page 160 of the same work, though no reference is made to the erroneous statement that preceded it:

> The genus *Orbiculoidea* of D'Orbigny was first defined and exemplified in the Frodrome de Paléontologie, vol. i, p. 44, the date of this work being 1850, not 1849. Dall is in error in stating that *Orbicula Morrisi*, Davidson, is the first species mentioned under the diagnosis quoted. D'Orbigny here gives three species in the following order: *O. Forbesi*, Davidson, *O. Morrisi*, Davidson, *O. Davidsoni*, D'Orbigny. As no species is specially designated as the type of the genus we are compelled to assume these three as types in their order and upon their merits. It is shown on page 136 that the first of these, *O. Forbesi*, Davidson, is unquestionably congeneric with *Schizotreta elliptica*, Kutorga, Kutorga's genus having been established in 1848. As this species, therefore, can not be used as the type of Orbiculoidea, we must assume the second species as the typical representative of the genus, and upon this is based the distinction throughout the foregoing pages in the use of this term *Orbiculoidea* by D'Orbigny and by Davidson. At the place cited in the "Prodrome" the date "1847" stands after the name of the genus. The explanation of its use appears upon page lix of the Introduction, and the date of publication of the work renders its adoption untenable.

The views expressed in the two paragraphs quoted from Hall and Clarke seem to me in some respects not quite consistent. In the first passage they say "Mr. Dall has observed that in neither the first

use of the term, nor in the second, was an example cited, and, therefore, reaches the unavoidable conclusion that *O. morrisi*, being the first citation made by the author, must be assumed as the typical species." Even if these expressions are to some extent a quotation from Dall, the authors manifestly concur in them. I read from this nothing else than that Hall and Clarke accept the "first species" rule in principle and also the application of the rule to the selection of a genotype for *Orbiculoidea*. What they say may be paraphrased in this wise: The first species cited by an author under a new genus must be assumed as the genotype (qualifying conditions of course being absent), and specifically, *Orbicula morrisi* being the first species cited by d'Orbigny must be assumed as the genotype of *Orbiculoidea*, both conclusions being unavoidable. In the second passage, however, they say that we are compelled to assume these three species as types in their order and upon their merits, that the first mentioned species is undoubtedly congeneric with *Schizotreta elliptica* and therefore can not be used as the type for *Orbiculoidea* (evidently because to do so would make *Orbiculoidea* a synonym of *Schizotreta*) and that, consequently, we must assume the second species as the typical representative of the genus. They appear, in this passage, not only to have forgotten their original conviction regarding the "first species" rule, but to have overlooked the fact that if Dall had already selected a genotype, they themselves had no subsequent choice. Indeed, they seem to have lost their grasp on the situation entirely. It will be worth our while to look into the facts about these critical points more closely than can be done through the passages quoted.

So far as I am informed the name *Orbiculoidea* was introduced by d'Orbigny in 1847.[4] It was again used by him in 1850,[5] and a third time in the *Prodrome*.[6] In each of these publications the genus was defined but only in the *Prodrome* were any species cited under it.

The earliest of these works was an outline of the classification of the Brachiopoda, and the definitions are rather brief. As the characters ascribed to the higher groups supplement those ascribed to the genera, it will be desirable to quote, in addition to the diagnosis of *Orbiculoidea*, that of the famly Orbiculidae in which *Orbiculoidea* was included. This family comprises four genera, *Siphonotreta*, *Orbicella*, *Orbiculoidea*, and *Orbicula*. Its distinctive characters are given as "an

[4] Acad. Sci. Paris, Comptes rendus **25**: 269. 1847.
[5] Ann. sci. nat. **13**: 351. 1850.
[6] *Prodrome de Paléontologie* **1**: 44. 1850.

exterior muscle issuing through the lower valve; shell free," and the distinctive characters of the genus *Orbiculoidea* are given as "shell corneous, not punctate (perforée), muscle pedunculate."

The description in the *Annals* is much more extended and detailed, occupying with the discussion an entire page. *Orbiculoidea* is thus described:

> animal attached (fixé); shell free, regularly corneous, suborbicular, inequivalve; lower valve concave, pierced in the deepest part by an elongated opening, which is simple and lateral to the beak, and through which must have issued a simple muscular pedicle attached only to the internal parts of the valve. Upper valve conical, with the beak eccentric.

There follows a rather lengthy discussion of the relationship, both as to resemblances and differences, of *Orbiculoidea* to *Orbicula* and then to *Orbicella*, but the discussion rather throws new light on the characters cited in the description than amplifies the description with new characters. The author adds, however, that 27 species of this extinct genus are known to him, of which the first are from the "étage Murchisonien," the greatest number from the Carboniferous, and the last from the Neocomian.

The diagnosis given in the *Prodrome* although considerably condensed is in substance the same as the diagnosis given in the *Annals*. It has already been repeated in this paper in the quotation taken from Hall and Clarke. Their quotation, however, is not exact, punctuation, diacritic marks, and even whole words having been changed, but the changes do not affect the sense except in one instance, the substitution of "ou" for "au," apparently a clerical or typographic error. To again quote this description would be superfluous but the fact will bear repetition that under it three species are cited and in the following order—*O. forbesi*, *O. morrisi*, and *O. davidsoni*. So much for d'Orbigny's three descriptions of the genus *Orbiculoidea*.

As every one knows, the modern conception of a genus, taxonomically at least, is that the genus centers in a typical species, the genotype, and the rule for a long time held wide acceptance that if the author of a genus did not name a genotype, as nearly all the early authors failed to do, any one subsequently might select as the genotype one of the species originally cited, though he was required to select the first of those species unless this procedure in some manner thwarted the obvious intentions of the author. Proceeding under this rule and applying it to the facts set forth above, I concluded in 1909, that *O. forbesi* was the genotype of *Orbiculoidea* and that *Orbiculoidea* d'Orbigny was a synonym of *Schizotreta* Kutorga.

Professor Prosser, however, takes exception to these conclusions on the ground that an amelioration of the "first species" rule had been adopted two years prior to the publication of my report in 1909, or about the time I was engaged in writing it. He says on this subject:[7]

> The above case is apparently covered by the rules reported by the International Commission on Zoological Nomenclature and adopted by the Seventh International Zoological Congress in August, 1907. Article 30, section g of these rules states that "If an author, in publishing a genus with more than one valid species, fails to designate or to indicate its type, any subsequent author may select the type, and such designation is not subject to change. (Type by subsequent designation.)" Section k of the recommendations to this article further says that "If some of the original species have later been classified in other genera, preference should be shown to the species still remaining in the original genus (Type by elimination)." From the above rule it appears that Hall and Clarke had the right to select a type for the genus *Orbiculoidea* and since they did, that species is the genotype and the genus *Orbiculoidea* stands.
>
> In place of *Orbiculoidea* Dr. Girty uses provisionally *Lingulidiscina* which was proposed by Whitfield (*Lingulodiscina*) for *Lingula exilis* Hall. It has not yet been conclusively shown, however, that Whitfield's genus *Lingulodiscina* is identical with d'Orbigny's *Orbiculoidea*. Dr. Girty states that *Orbiculoidea newberryi*, which Schuchert in his synopsis of American fossil Brachiopoda referred to the genus *Lingulodiscina*, "is certainly a member of the *Orbiculoidea* group." This may have strengthened his opinion that these two genera are identical; but the reference of *O. newberryi* to *Lingulodiscina* was accidental, as is shown by the following quotation from a letter by Professor Schuchert to the writer, dated July 2, 1906: "Certainly I have made a mistake regarding *Orbiculoidea newberryi*, for it is not a *Lingulodiscina*. It is an error I can not account for. When I received your letter I remembered the species as a *Orbiculoidea*. Please make the change and I thank you for directing my attention to it."

The facts advanced by Professor Prosser undoubtedly are important and give a new turn to the *Orbiculoidea* question, bearing as they do upon both of its phases, the proper application of that name and the availability of *Lingulidiscina* to replace it. So far as they bear on the present phase, however, the validity of *Orbiculoidea*, I believe that they really lead to a conclusion quite different from his.

My objections to Professor Prosser's argument travel along two distinct lines. It is of course tacitly understood that no rules of nomenclature in biology can be actually enforced, that they are binding only as they appeal to the good judgment and impartiality of those who work in that field, but we may assume that the "first species" rule and the two rules cited by Professor Prosser, which in a measure supersede it, are on those grounds really binding on every one. If

[7] CHAS. S. PROSSER. *The Devonian and Mississippian formations of northeastern Ohio.* Bull. Ohio Geol. Surv. (4) 15: 203. 1912.

that is so, the rigid "first species" rule was binding on every one up to 1907, when the new and amended rules came into force. I agree with Professor Prosser in the general thesis that I, publishing in 1909, was bound by the rules of nomenclature adopted in 1907, and I would expect him to agree with me that Hall and Clarke, publishing in 1892 were equally bound by the rules of nomenclature in force at that time. But the rule of nomenclature in force in 1892 was the strict "first species" rule and I am unable to see how Hall and Clarke "had the right" in 1892 to select the second species by virtue of an amendment that was not adopted until fifteen years later, in 1907; yet this is what Professor Prosser seems to say. Nor, I think, could the claim be advanced that a genotype selected by Hall and Clarke in violation of one part of the rule was established for ever by another part of the same rule which states that such designation is not subject to change.

The crux of the situation, however, is found in a fact that Professor Prosser seems to have overlooked, a fact that lies at its very core and that diverts from my conclusions to his own the adverse force of the rule that he invokes. He says namely "from the above rule it appears that Hall and Clarke had the right to select a type for the genus *Orbiculoidea* and since they did, that species is the genotype and the genus *Orbiculoidea* stands." It appears to me, on the contrary, not only that Hall and Clarke had no right to select the particular genotype that they did select, but that they had no right to select a genotype at all because Dall had already selected one in 1877.

At this point it is necessary to consider what author first selected a genotype for *Orbiculoidea*, and what genotype he selected. It is clear from Dall's comments that he did not know of any selection prior to his own, and it is clear from what Hall and Clarke have written that they also did not know of any prior to it. Dall's selection, therefore, appears to be the first and therefore the determining one. The only possible exception lies in the fact that Davidson, who regarded *Orbiculoidea* and *Schizotreta* as congeneric, employed *Schizotreta elliptica*, the type species of Kutorga's genus, as if it were the type species of d'Orbigny's. It is doubtful if Davidson's course has any direct bearing on the present discussion, especially as *S. elliptica* was not one of the species originally referred to *Orbiculoidea* by d'Orbigny nor indeed was it mentioned by him in any way.

The discussion must now be addressed to the question what species is to be considered as having been selected by Dall as the genotype of *Orbiculoidea*.

This work of Dall's to which reference has already several times been made, is entitled "Index to the Names which have been Applied to Subdivisions of the Class Brachiopoda," and the author apparently sets out to list all the names that had been employed for brachiopod genera, to fix or crystallize the meanings of those names by determining their typical species and to suggest the relations of the genera to one another. To many of the names he has appended notes of poignant significance and to some rather lengthy discussions. Where the selection of a genotype fell to him he seems invariably to have followed the "first species" rule. No exceptions to this practice were noted though for obvious reasons only a relatively small number of genera were canvassed. As touching *Orbiculoidea* he refers to the *Comptes Rendus* and to the *Annals* with the remark "No examples cited" and follows mention of the Prodrome with the comment:[8] "First species *O. Morrisii*, 'd'Orbigny 1847' * * *. Two other species cited as of 'd'Orbigny 1848.' It would appear as if *O. Morrisii* must be considered as the type." That *O. forbesi* is really the first species cited under *Orbiculoidea* and not *O. morrisi* has already been pointed out.

From the expressions here employed by Dall, and from the circumstance that he mentions *O. morrisi* as the "first species" though it is in fact the second species cited in the *Prodrome*, one might infer that Dall had in mind not the sequence of names in that publication but rather the date affixed to *O. Morrisi* by d'Orbigny, 1847, whereas the two other species are cited as of d'Orbigny 1848. The date for *O. morrisi*, however, is an obvious misprint for 1848, so that as regards date of citation under *Orbiculoidea*, all three species are on the same level. The scheme of citation employed in the *Prodrome*, as explained on page LIV of the introduction, is this: Under the genus is given first the name of the species, then the name of the author who referred it to its accepted genus with the date of transfer, then the citation in its original form followed by the name of the original author and the date of publication. In this instance the citation takes this form, under *Orbiculoidea*—"315. *Morissii*, d'Orb., 1847. *Orbicula Morissii*, Davidson, 1848" with the book reference and place of discovery. It is obvious at a glance that d'Orbigny could not have referred this species to *Orbiculoidea* in 1847 before it was described by Davidson in 1848. The probability that the date 1847 is a misprint instead of a reference to some transaction that no one has as yet noted, is enhanced

[8] W. H. DALL. *Index to the names which have been applied to the subdivisions of the Class Brachiopoda.* Bull. U. S. Nat. Mus. **8**: 51. 1877.

by the abundance of errors in this work, no less than three occurring within the narrow limits of these three citations. The three species referred to *Orbiculoidea* in this place were all described by Davidson under the same genus, in the same report, and of course under the same date, 1848. That d'Orbigny cites them as having been also transferred to the genus *Orbiculoidea* in 1848, is explained by the fact that he expected the *Prodrome* in which the transfer was made (and from which I am quoting) to have been published on that date as already mentioned. The date of transfer of all three species should, of course, be changed to 1849 if not to 1850, but 1847 as printed is clearly a mistake for 1848. This is one error; *Morrisii*, the name of the species, is misspelled *Morissii;* and a third error is found in the species cited as *Orbiculoidea Davidsonii*. This name was proposed in the *Prodrome* (as if in 1848) in substitution for "*Orbicula Koninckii*" Davidson 1848 (non Geinitz 1848). Davidson's species, however, was not called *Orbicula Koninckii* but *Orbicula Verneuilii*, confusion on my part being impossible because d'Orbigny gives the place of original description. Beyond reasonable question, then, the date 1847 for the transfer of *Orbicula morrisi* to *Orbiculoidea* was a misprint for 1848. All three species were transferred at the same time in the *Prodrome*, whose date should apparently be taken as 1850.

Dall was probably less liable to error in these matters than most of us, but in this instance he clearly erred in one respect or the other. He intended to name the "first species" cited under *Orbiculoidea* as the type of that genus; the fact that he actually named *O. morrisi* is capable of but two explanations. He may have been fully aware that all three species had simultaneously and for the first time been cited under *Orbiculoidea* in the *Prodrome*, but by inadvertence have mentioned the second of these instead of the first; or he may have been misled by the date appended to *O. morrisi* into the belief that that species had been transferred to *Orbiculoidea* before the two others. As regards the latter possibility, d'Orbigny's error was on the face of things so palpable in view of the year in which the species *morrisi* was published, that Dall could not have failed to notice it, and in addition he had so immediately set down the *Comptes Rendus* as the only publication by d'Orbigny in 1847 relating to the genus *Orbiculoidea* with the remark "No examples cited," all this within the space of about four lines, that he could not have interpreted the date 1847 as referring to some transaction which was at the same time unnoted and essential to note. That Dall had in mind the sequence of species in the *Pro-*

drome and that intending to name *O. forbesi* as the "first species," which in fact it was, he carelessly named *O. morrisi*, is by far the more probably conclusion. Thus, on the one hand it is fairly certain that Dall intended to name *O. forbesi* as the typical species of *Orbiculoidea;* on the other hand, it is quite certain that he named *Orbiculoidea morrisi*, apparently an act of inadvertence. The question is squarely put: the species that Dall intended to name or the one that he actually did name? If there were any doubt in my mind as to Dall's intentions, or if his intentions followed any but the sole legitimate course, and his own consistent practice, I would hesitate, but my own judgment is clear that *O. forbesi* should be regarded as having been selected by Dall as the typical species of *Orbiculoidea.* Indeed, if the "first species" rule is accepted as applying to all cases down to 1907, I do not see how *O. morrisi* could have any standing on the basis of Dall's performance.

My understanding of the "first species" rule is that the International Commission did in mass what they could not do in severalty—select the first species of all undesignated genera as genotypes. Individual workers were merely commissioned to specify the first species as each of those genera came up for revision. If they failed to name the first species, except for some valid reason, such as that to do so would thwart the original author's intention, their action was without authority and without effect.

On this understanding, if Dall's selection of a genotype for *Orbiculoidea* was the first species—as I think he intended it to be—then *O. forbesi* is the genotype. If, however, his selection was the species which he accidentally named, *O. morrisi*, his selection was void and the genus still remains undesignated so far as he was concerned. If this is true of Dall's selection it is equally true of Hall and Clarke's. *Orbiculoidea* then continued to be without a specified genotype until 1909, when I treated it as if typified by *O. forbesi*, in obedience to the first species rule. But by that time the first species rule had been modified in the terms already quoted from Prosser. Prosser's application of the modified rule to Hall and Clarke's discussion is obviously beside the mark; the modifications could not possibly apply to something that was done 15 years previously. They would apply, however, to what I did in 1909, and what I did was in some measure an infraction of them inasmuch as I used the first species as the genotype, whereas the rule reads that if some of the species have later been classified in other genera, preference should be given to the species still remaining

in the original genus. This, it will be remarked, is not an injunction but a recommendation, but if it is thought to invalidate my use of *O. forbesi* as the genotype of *Orbiculoidea*, *Orbiculoidea* would appear to be without a valid genotype even at the present writing. In my judgment, Dall's selection should be regarded as the determining one and he should be regarded as having selected the "first species," *O. forbesi*.

The year from which *Orbiculoidea* takes its date is not beyond our present concern. The genus was undoubtedly described in 1847, whereas the genus *Schizotreta* was not described until 1848. If the genera are accepted as established on those dates and if they should prove to be synonymous, then *Schizotreta* would yield to *Orbiculoidea*, not *Orbiculoidea* to *Schizotreta*. Dall, Davidson, and d'Orbigny date *Orbiculoidea* from 1847; Hall and Clark use 1850. As the being of a genus, according to modern ideas, is centered in a typical species, a generic name can not be accepted as established until species are named under it, one of which by selection, either original or subsequent, becomes its genotype. A bald generic description does not constitute validity. If this is so, *Orbiculoidea* is validly dated only from the *Prodrome*, whereas *Schizotreta* is validly dated from 1848. But the year from which the *Prodrome* should be dated is itself somewhat doubtful.

The description of *Orbiculoidea* in the *Prodrome* occurs in the first volume, and the copy of that volume now before me bears the imprint of 1850. The third volume, by the way, is dated 1852. Dall dates the *Prodrome*, at least the first volume and consequently the genus *Orbiculoidea*, as of 1849. Hall and Clarke cite the same date in the first quotation from their monograph, but cite 1850 in the later quotation. The adoption of the date 1849 appears to be explained by a remark found on page 59 of the introduction to the *Prodrome*, where d'Orbigny says that he completed the work in 1847 and expected to publish it in 1848 but owing to political circumstances, he was unable to bring it out before 1849. That d'Orbigny was disappointed in the hope of publishing his work in 1849 seems much more likely than that it was actually published in 1849 with the false imprint 1850. Unless other facts are brought to light the first volume of the *Prodrome* and the date of the genus *Orbiculoidea* should be taken as of the year 1850. On these grounds I reach the conclusion that *Schizotreta* has priority over *Orbiculoidea*, that the type species of *Orbiculoidea* is *O. forbesi*, and that if *O. forbesi* is a *Schizotreta*, *Orbiculoidea* becomes a synonym.

Of the premises which seemed to justify me in diverting d'Orbigny's name *Orbiculoidea* from the group of species to which Hall and Clarke wished to apply it, several have already been considered. I have attempted to show that the first valid description occurs in the *Prodrome*, and that the *Prodrome* was not published until 1849, or more probably 1850, so that *Schizotreta* has priority over *Orbiculoidea*. The various attempts to select a genotype have also been discussed together with their standing under the rules of nomenclature governing such selection, with the result that Dall's selection was found to be the first and that, in pursuance of his obvious intention, it seemed best to regard the first species, *O. forbesi*, as the genotype of *Orbiculoidea*. It remains to consider whether *Orbiculoidea* and *Schizotreta* are distinct genera or subgenera, and whether *O. forbesi* is a *Schizotreta* or an "*Orbiculoidea*" as those names were employed by Hall and Clarke.

In my original discussion of *Orbiculoidea* I accepted Hall and Clarke's dictum that *Schizoıreta* and "*Orbiculoidea*" were distinct genera, or as they estimated values, subgenera. On this head those authors say (p. 136) that *Schizotreta* Kutorga

> may very well stand to include those forms essentially in agreement with *Orbiculoidea*, d'Orbigny, but having thicker shells and the relative convexity of the valves reversed, bearing, in fine, the same relation to d'Orbigny's genus as *Strophonella* to *Strophodonta*, among the articulate brachiopods.

Several additional characters of possible value in distinguishing the two genera are suggested by a reading of Kutorga's description. He says namely that *Schizotreta* is marked by fine radial striae (radial-leistchen) and that the shell is not phosphatic (keineswegs hornartig) but like that of the other Siphonotretacaeae. The radial striae may, however, be only such irregular, almost casual lines as may be seen on some of our common Paleozoic discinoids. Furthermore, though I would interpret "hornartig" as refering to that shiny phosphatic appearance which is characteristic of discinoid shells in their fossil state, this interpretation is possibly a mistake, for the shell substance of *Siphonotreta* is described by Hall and Clarke as calcareo-corneous and apparently not different from that of *Orbiculoidea*. Therefore, the distinction suggested by Kutorga's term "hornartig," even if correctly understood, may be only a matter of preservation. Let the distinction between these genera then rest merely on the relative convexity of the valves and the thickness of the shell.

In the type species of *Schizotreta* the brachial valve is depressed convex, more often flat, and the pedicle valve strongly conical, just the re-

verse of the relation common in *"Orbiculoidea."* On these grounds I am fain to accept the judgment expressed by Hall and Clarke that *Schizotreta* and *"Orbiculoidea"* may advantageously be distinguished but I can not grant that the distinction between them is on a parity with that between *Strophodonta* and *Strophonella.* Passing over the fact that the distinction between those articulate genera only comes about in the later growth stages, we must nevertheless recognize that the reversal in curvature of the valves of *Strophonella* is such that though both genera are concavo-convex, in *Strophodonta* the pedicle valve is convex and brachial valve concave, whereas in *Strophonella* the pedicle valve is concave and the brachial valve convex. In the two discinoid genera, on the other hand, both valves are convex. Neither is the pedicle valve in *"Orbiculoidea"* concave as it is in *Strophonella,* nor is the brachial valve in *Schizotreta* concave as it is in *Strophodonta.* The curvature of the valves is not reversed, merely their relative convexity. The difference between *Strophonella* and *Strophodonta* may be of the same character only carried further. It is, however, carried much further. The difference between *Schizotreta* and *"Orbiculoidea"* would appear more aptly compared to that between *Rhipidomella* and *Schizophoria* or perhaps between *Dalmanella* and *Schizophoria.* In those orthoid types, however, the difference in configuration is supported and emphasized by accompanying differences in the size and shape of the muscular imprints.

Let us, however, accept Hall and Clarke's conclusion that *Schizotreta* and *"Orbiculoidea"* are valid groups distinguished by the characters set forth above, and turn to the question whether *Orbiculoidea forbesi* is a *Schizotreta* or an *"Orbiculoidea."* Hall and Clarke are very positive on this point and the conclusions that I reached in 1909 were in great measure due to an acceptance of their opinion without a personal investigation of the facts. This was ill done. To quote from Hall and Clarke—they say on p. 129 that Davidson's description of *Orbicula morrisi* supplemented by McCoy's show it to be in precise harmony with the paleozoic discinas generally, "while those species now passing under the name of *Orbiculoidea,* Davidson, are distinctive in having the relative convexity of the valves reversed, the pedicle valve being the more convex." Whether this is generally true or not, is of little moment; our concern is only with *O. forbesi* which is one of Davidson's orbiculoideas. Of this more anon. Again, on p. 136, they say, "It thus appears that there is no essential difference in *Schizotreta,* Kutorga and *Orbiculoidea,* Davidson," and on p. 160 that *"O. Forbesi,* David-

son, is unquestionably congeneric with *Schizotreta elliptica*, Kutorga." This is said to have been "shown" on p. 136, but *O. forbesi* is not mentioned on that page and the only thing that can in any sense be regarded as shown there is that all these types have the pedicle valve constructed on essentially the same plan, *Schizotreta* Kutorga as based on *S. elliptica*, *Orbiculoidea* of Davidson (as including if not typified by *O. forbesi*), and authentic *Orbiculoidea* d'Orbigny (as typified, according to Hall and Clarke, by *O. morrisi*).

In view of these repeated assertions, one is somewhat surprised to find Davidson both describing and figuring *O. forbesi* with the upper or brachial valve more convex than the pedicle valve. This is true whether one consults the original publication in French, appearing in 1848, or his later and better known *Monograph on British Brachiopoda*. The pedicle valve may be rather more convex than the pedicle valve of our orbiculoideas commonly is, though not abnormally so, but it is distinctly less convex than the accompanying brachial valve. If *Schizotreta* and *Orbiculoidea* are distinguished only in the manner suggested by Hall and Clarke, *O. forbesi* is an *Orbiculoidea* and not a *Schizotreta*. Davidson, it will be recalled, regarded *Schizotreta* as identical with *Orbiculoidea* and because he accepted 1847 as the date of publication for *Orbiculoidea*, he employed d'Orbigny's name in preference to Kutorga's. In discussing the brachiopod genera recognized in his monograph, he uses a figure of *Orbiculoidea elliptica* as representative of the genus *Orbiculoidea*, *Orbiculoidea elliptica* being of course Kutorga's species *Schizotreta elliptica*, typical of that genus. Davidson's figure, however, shows a large shell in which the upper valve is distinctly more convex than the lower, a shell markedly different in both respects from authentic *S. elliptica* of Kutorga and apparently a quite distinct species. Davidson's *Orbiculoidea elliptica* seems much more naturally associated with his *O. forbesi*, likewise a large shell, and if any distinction is to be made among them, *O. forbesi*, *O. morrisi*, and *O. elliptica* Davidson non Kutorga would seem to belong in one group and *Schizotreta elliptica* Kutorga in another, pretty much as d'Orbigny arranged them, though he took no cognizance of Kutorga's work nor of Davidson's identification of *O. elliptica*, which came later. If Davidson's identification of *Orbiculoidea elliptica* be accepted, then apparently *Orbiculoidea* and *Schizotreta* are the same thing, for his figure has every appearance of being a true *Orbiculoidea*. They are also the same thing if *O. forbesi* is admitted into the genus *Schizotreta*, as Hall and Clarke state positively that it should be, for the

distinction that they try to make between the two genera becomes impractical if not fanciful in as much as *O. forbesi* differs less from the normal *Orbiculoidea* in the relative convexity of the valves than from typical *Schizotreta*. In either contingency *Schizotreta* and *Orbiculoidea* would appear to be synonymous, a relationship that has been maintained by a number of authors; and if the two names do cover essentially the same types of structure and configuration then I believe that *Schizotreta* should be retained in preference to *Orbiculoidea* as having had a prior valid description. The evidence seems, however, rather to indicate on the one hand that *O. elliptica* of Davidson is not the same species as *Schizotreta elliptica* Kutorga or even congeneric with it, and on the other that *Orbiculoidea forbesi* is a true *Orbiculoidea*.

Thus the two errors that I attribute to Hall and Clarke cancel each other. The type species of *Orbiculoidea* is not *O. morrisi* as they wished to make it, but *O. forbesi*. *O. forbesi* on the other hand is not a *Schizotreta* as they affirmed but a true "*Orbiculoidea*." My previous conclusions, reached by accepting one of these premises and rejecting the other now seem to me untenable. *Orbiculoidea* now seems properly employed for the group of species covered under it by Hall and Clarke, though even the premises relied on for this conclusion are liable to revision. Where, as here, the evidence available is so largely comprised in books rather than in specimens one is especially liable to misconceptions and misjudgments, for one is dealing with part of the evidence only and with facts as they are transformed by the lenses of so many different minds.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

BIOLOGICAL SOCIETY

712TH MEETING

The 712th meeting was held in the assembly hall of the Cosmos Club December 3, 1927, at 8:10 p.m., with President GOLDMAN in the chair and 82 persons present. New member elected: F. C. BISHOP.

TITUS ULKE: *Flora of Yoho Park* (illustrated).—The speaker gave an account of his work during several summers in Yoho Park, British Columbia, illustrated by slides showing characteristic Alpine scenery and many flowers in colors. The rocks almost throughout the park are calcareous shales. Three life zones are represented, the Arctic-Alpine, the Hudsonian, and the Canadian.

G. F. SIMMONS, Cleveland Museum of Natural History: *Natural history notes from the cruise of the "Blossom" in the South Atlantic* (illustrated).—

The speaker described his experiences on the natural history cruise of the "Blossom" to western Africa and various islands of the south Atlantic, making particular mention of the birds and flora of the islands visited.

713TH MEETING

The 713th regular meeting was held in the assembly hall of the Cosmos Club December 17, 1927, at 8:10 p.m., with President GOLDMAN in the chair and 115 persons present. New members elected: ELMER HIGGINS, G. F. SIMMONS, R. O. SMITH

F. C. LINCOLN reported the recovery of an Arctic Tern, banded in Labrador in 1927, at La Rochelle, France. This is the first time an American banded bird has been taken in Europe.

S. F. BLAKE read from a Manila newspaper a notice of the shooting of a swallow, banded in Japan, in the Philippine Islands.

PAUL G. REDINGTON: *Informal discussion of some biological problems.*—The speaker illustrated the problems presented to the Biological Survey for solution by discussing the question of placing a close season on woodcock. Evidence as to the increase or decrease of this bird under present conditions is conflicting and there is neither time nor funds for a thorough survey of actual conditions by trained ornithologists. Under the circumstances, the only practicable method of obtaining information has been the circulation of questionnaires, and the evidence derived from these points unmistakably to the general decrease of the bird in its range as a whole, although it may be holding its own in some localities. The question of tularaemia in wild animals other than rabbits was briefly referred to. The migration of caribou during the past summer near Fairbanks, Alaska, has been the greatest ever known in that region, the number of animals being estimated at 500,000 to 750,000. In discussion, CHARLES SHELDON stated that he doubted whether this migration indicated a recent great increase in numbers, and was rather of the opinion that excessive numbers in this region were due to circumstances of migration. E. P. WALKER concurred in this belief.

JOHN M. HOLZWORTH: *Motion pictures of mountain sheep, mountain goats, caribou and other big game from Alaska and Idaho* (illustrated).—The speaker showed a remarkable series of pictures taken in the region of the Salmon River, Idaho, in the Smoky River Country, in the Yukon, and elsewhere in the Northwest and gave an interesting commentary on the pictures as they were shown.

S. F. BLAKE, *Recording Secretary.*

SCIENTIFIC NOTES AND NEWS

The Smithsonian Institution has awarded the Walter Rathbone Bacon research fellowship for the years 1928–1930 to Dr. Paul Bartsch, curator of mollusks in the National Museum. Dr. Bartsch will make use of the award to collect material for the completion of a monograph on the land shells of the West Indies. The fellowship, established under the terms of the will of Mrs. Virginia Purdy Bacon of New York, became available in 1924. It is given for two years and may be extended; a report is made to Smithsonian and all collections, photographs, records and equipment become the property of the Institution.

Professor H. H. BARTLETT of the University of Michigan spent two weeks at the National Herbarium working on his Sumatran collections. The grasses have been identified at the Grass Herbarium. Most of the other phanerogams are to be determined by Dean E. D. Merrill of California, and the ferns by Dr. E. B. Copeland.

B. S. BUTLER of the U. S. Geological Survey will give a series of lectures on mining geology at the University of Arizona, and will be on leave of absence from the Survey until the end of May.

A. C. SPENCER has gone to Socorro, New Mexico, for a stay of several months to complete his work on the Santa Rita district. His headquarters will be at the New Mexico School of Mines, at Socorro.

Dr. R. S. BASSLER, Curator of Stratigraphic Paleontology in the National Museum, was recently elected Secretary of the Paleontological Society of America for the 20th year.

BAILEY WILLIS, President of the Geological Society of America, has recently been in Washington for conference on scientific matters.

Prof. C. K. LEITH of University of Wisconsin visited Washington en route to New York and the West Indies.

Dr. E. C. ANDREWS, Government Geologist of New South Wales, addressed the Geological Society of Washington, and less formally the geologists of the U. S. Geological Survey, on the geology of the Broken Hill district, Australia, and the tectonics of the Pacific.

Prof. DAYTON C. MILLER, of the Case School of Applied Science, Cleveland, Ohio, gave an experimental lecture, *Photographing and analyzing sound waves*, before the ACADEMY February 16. The general nature of sound and sound waves was discussed, and a detailed explanation of noise and tone, and of pitch, loudness and tone-quality was given. A method for obtaining photographic records of sound waves was described, and numerous photographs were shown. By means of the "Phonodeik," "living" sound waves from the speaker's voice, from various musical instruments, and from singing and whistling were projected on the screen.

The Ore Deposits Club met at the Geological Survey on February 14. T. S. LOVERING presented a paper by B. S. BUTLER and W. S. BURBANK, of the Colorado office of the Survey, on *The relation of electrode potentials of some elements to the formation of hypogene ore deposits.* There was general discussion of the question whether the solutions given off by a magma are acid during the early stages of ore-deposition. The evidence on this point is inconclusive.

DONALD C. BARTON, consulting geologist, of Houston, Texas, lectured at the Interior Department on February 16, on *The use of the torsion balance in geophysical prospecting.*